

test against Herbert Spencer's application of the principles of evolution to the solution of vital, social, and mental problems. The author then proceeds to set up a ghost founded on the statement made some years ago, that "there is no evolution without spontaneous generation." To refute the theory of spontaneous generation will be, he says, to give a direct blow to the theory of evolution. This, he maintains, has been amply done by Pasteur and others, and a number of the most important experiments are here referred to.

The author proceeds to argue that, since evolution has failed to explain the first beginnings of life, there must have been a God who created matter, a living germ, and an intelligent mind, and that the three creations were distinct.

He gives a clear account of many of the vital as distinct from the non-vital processes, and draws especial attention to the fact that solutions of many of the higher organised products polarise light, and that the only organic bodies which have been formed synthetically are the lower organised products which do not polarise light. It is doubtful, however, whether the distinction is one which will hold much longer, as chemical methods are constantly improving.

The author adduces no new facts, but he has the merit of bringing together in a very readable form, statements more or less scattered about in several books and periodicals.

*History of the Royal College of Surgeons in Ireland, &c.* By Sir Chas. A. Cameron. (Dublin: Fannin and Co., 1886.)

THIS volume, which is published at the expense and by the authority of the College of Surgeons, collects together the charters and histories of the various Irish Medical Schools and Colleges, and supplies biographies of the leading members of the medical profession in Ireland, together with a list of their works.

Many curious ana are given of the old physicians; among others, of Joseph Rogers (1734), one of the first to feed fevers, who gave a patient daily for a month four to six quarts of sack-whey and two quarts of mulled canary, which was certainly vigorous treatment.

The first Society for the regulation of medicine in Ireland dates back to 1446, when Henry VI. established a Guild of Barbers in Dublin; and later on, in 1572, Queen Elizabeth granted a new charter by which women were admissible to the guild; and in those days a barber was equivalent to our surgeon. This Society lingered on until the foundation of the College of Surgeons in 1784.

This book will be of great use as a work of reference with regard to the state of medicine at any period in Ireland, and its compilation must have been a laborious labour of love on the part of the author. The biographies, which are very numerous, form the most interesting part of the work, and include a large number of world-renowned names, the greatest of which are probably Graves and Stokes.

#### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

#### Physiological Selection and the Origin of Species

As I was unable to be present at the Linnean Society when Mr. Romanes read his paper on the above subject, I may take the opportunity furnished by the publication of the abstract in

these columns to put forward certain views which I have long held with reference to the points raised by the author. I may remark that I am writing under the disadvantage of distance from notes or books of reference, and that I have not yet seen the complete paper. Moreover, my work of late years has run off biological tracks, and I can but regret that my remarks must, under the present circumstances, be of a more or less general character; but at any rate they may be of use as a contribution to the discussion which Mr. Romanes' carefully considered paper well merits at the hands of biologists.

In the first place, I should like to point out that evolution by what Mr. Romanes calls "independent variation," or the prevention of crossing with parent forms, is very ably discussed by Weismann in one of his earlier works, "Ueber den Einfluss der Isolirung auf der Artbildung" (1872), which essay I can commend to the notice of all interested in the subject. Weismann termed this principle "*Amixie*," and for want of a better word I have rendered this "*Amixia*" in my edition of the "Studies in the Theory of Descent," in which work the principle is also frequently alluded to.

All evolutionists will agree with Mr. Romanes that natural selection *per se* is incompetent to account for the origin of species. This has long been admitted by naturalists, and Darwin himself in later life frankly acknowledged that in the early editions of the "Origin of Species" he over-estimated the power of this agency. Nevertheless, Darwin to the last considered natural selection as the *chief agency* in the evolution of species, and no one saw more clearly than he did the difficulties which surrounded the formation of incipient species, owing to the obliteration of new characters by intercrossing with the parent form. The sterility of natural species as compared with the fertility of domesticated races is also a difficulty which Darwin fully recognised and did much towards meeting. The results of his investigations in this direction have been to break down the supposed fixity of the rule, although it must be admitted that the broad fact still remains, and we cannot but be grateful to Mr. Romanes for once more emphasising this difficulty with his characteristic clearness. It is chiefly—if not entirely—with the object of meeting this difficulty that "physiological selection" has been conceived, because, as it appears to me, the other difficulties referred to by Mr. Romanes, viz. those connected with the prevention of intercrossing and the inutility of trivial characters, are quite subordinate to this main difficulty, and need not be further considered until the admissibility (or otherwise) of physiological selection has been settled. The questions now to be decided are whether natural selection + sexual selection + correlated variability + amixia + use and disuse, &c., is really a theory of the origin of *species*, or whether these factors have been only made to "pose" as such? Is "physiological selection" competent to account for the origin of species?

If I interpret Mr. Romanes correctly, his theory is equivalent to the admission that amixia may become inter-racial, *i.e.* that it may arise among the individuals of a species without the intervention of physical barriers by the spontaneous origination of a physiological barrier, *i.e.* by variation in the reproductive capacity. That such a form of variation may exist I have long been willing to admit, and I do so now with all the more readiness in face of the arguments so skillfully marshalled by the author of the new theory. But, since Mr. Romanes admits the efficiency of natural selection, the question seems to resolve itself into this: Can physiological selection work independently of natural selection? If not, natural selection must still be regarded as a prime factor, and if physiological selection cannot originate a species independently of the control of natural selection, surely the latter, with its subordinate factors (of which physiological selection may be one), is still the chief element in the theory of the origin of species.

Let us suppose, for the sake of argument, that among the individuals of a species there arise certain varieties which are fertile *inter se*, but sterile with the parent form. There would thus arise a new race which could not be swamped by intercrossing with the predominant form, and the one species would practically be resolved into two—the parent form being still in the ascendancy as regards numbers. But the competition is always most severe between the most closely related forms, and unless the new form (arising by inter-racial amixia) possessed some distinct advantage over the old one, it would as surely be exterminated by the overwhelming majority of the parent type as it would be by intercrossing in the absence of amixia. Physiological selection thus appears to me to be as subordinate to

natural selection as sexual selection, correlated variability, the law of homology, or any other of the Darwinian factors. The expression used by Mr. Romanes for his new factor—the “Segregation of the Fit”—seems to imply fitness for something, presumably for the conditions of life, and if the survival of the “fit” race is determined by natural selection, then I venture to think that natural selection must still be regarded as *the* theory of the origin of species and as something more than a theory of the origin of adaptations.

To the foregoing Mr. Romanes will probably say that physiological selection is a *necessary* adjunct of natural selection, and that no new species can arise without the co-operation of his factor. If this be the case, then, bearing in mind the views which I have expressed with reference to the subordination of physiological to natural selection, it seems to me that the most likely course to pursue is to appeal to the latter for an explanation of the “primary” specific character, viz. sterility. It is true that Darwin and many of his followers have attempted in vain to account for this primary specific distinction by natural selection, but I still venture to think that the solution lies in this direction. Indeed, the elements of the solution appear to me to be furnished by the original theory of Darwin and Wallace, as I will now briefly attempt to show, hoping to elaborate the idea on some future occasion, or, still better, leaving it for development or extermination in the hands of professed biologists.

The struggle for life being the most severe between the most closely allied forms, diversity is in itself an advantage, because the individuals which depart from the parent type may (but not necessarily *must*) “seize on many and widely diversified places in the economy of nature, and so be enabled to increase in numbers.” Hence Darwin’s principle of “divergence of character,” so well restated by Mr. Romanes in his paper. Now, if diversity is an advantage, natural selection can deal with it like any other advantageous character, and *would seize upon any means afforded to secure its perpetuation*, provided always that the divergence was in the direction of some unoccupied “place in the economy of nature.” This last condition amounts to nothing more than that the divergence is to be of an advantageous character. Among the most effectual means of securing permanent diversity must be sterility with the parent form: hence, given a variability in the reproductive capacities of different pairs of individuals (which I have already conceded), the question is whether natural selection could not develop out of this more or less imperfect fertility a more or less complete sterility. If it is to the advantage of some particular variety not to resemble its parent form, out of the immense number of divergences which are always taking place (by ordinary variation) those varieties which showed the greatest infertility with the parent form would in the long run survive, for the very reason that their progeny, tending to inherit the characters of their parents, would possess the advantageous characters of the latter, which led to their survival in the first instance, and, among these characters, that diminished fertility with the parent form which lessens the chance of their extermination by intercrossing.

As the foregoing argument is necessarily expressed in general terms, it will be of use to specialise our ideas by an appeal to a hypothetical case. Suppose, for instance, that a dominant species gives rise to the twelve varieties A, B, C, . . . L, out of which four, B, D, K, L, possess some slight advantage over the parent form, adapting them to some new conditions in their environment. The four varieties thus stand a chance of surviving, while the eight others would be the “unfit.” Now these four varieties in their incipient stage (and in the absence of isolation) would be subject to extermination by intercrossing in the next generation with the parent form. But the chances against these four varieties being *equally fertile* with the parent form must be exceedingly great: let us suppose, therefore, that B and K are less fertile with the parent form than D and L. Under these circumstances the latter would be wiped out by intercrossing, while the former would tend to retain their peculiarities and thus survive. The peculiarities both of B, K, and D, L, were originally advantageous, but those of B, K, are alone allowed to survive. The parent species has, as it were, attempted to give rise to four derived species, and has only succeeded in producing two. It is a case of selecting out of a number of advantageous modifications those particular varieties which are the least fertile with the parent form. From the slight sterility thus produced in the initial stages, natural selec-

tion, by acting in the same direction, might evolve the more or less perfect sterility which we now behold, because every departure on the part of the derived form in the direction of fertility with the parent form would be a retrograde step tending to obliterate those advantageous characters which led to the first survival of the new form, and the descendants of such partially fertile departures would constantly be weeded out owing to the dilution of their peculiarities with the less advantageous characters of the parent form. To put the case in another way, it may be said that natural selection is constantly endeavouring to develop the most advantageous modifications of every species, but succeeds the better the less the degree of fertility of the advantageous modification with the parent form, and succeeds only perfectly by producing perfect sterility with the parent form. Fertile advantageous modifications, on the other hand, would be swamped by absorption into the parent form.

I thus venture to think that the theory of natural selection as sketched out by Darwin and Wallace is still a theory of the origin of species. The production of the sterility of species by this agency is, according to the present views, to be referred to the same causes as all the other modifications produced thereby, viz. the natural selection of a “spontaneously” occurring variation in the function of one particular organ. In the case of domesticated races no such selection with reference to the functions of the reproductive system has been effected, but the varieties have only been kept from interbreeding by what amounts to isolation. It is not surprising, therefore, that such artificial forms, which have been selected only with reference to *external characters*, should be fertile *inter se*, while natural species, in which fertility *inter se* has been rigorously suppressed by natural selection through long series of generations, should exhibit a greater or less degree of sterility.<sup>1</sup> In other words, “physiological” appears as one particular phase of natural selection, and as far as we can see there is no reason why there should not be other modes of variation leading to the same result by acting indirectly upon the reproductive system. But all such modes of variation would still be subject to development or suppression by natural selection. R. MELDOLA

Greenock, August 21

THE Duke of Argyll’s letter about organic evolution, published in your last week’s issue (p. 335), calls for a few remarks, as it is very misleading, and bespeaks some misconceptions on the part of the writer. He has evidently read his own views into the two articles on organic evolution contributed by Spencer to the April and May numbers of the *Nineteenth Century*. In those articles Spencer makes no “admission”; what he says there with respect to natural selection has been held by him for the last twenty-six years. He does not deny that the natural operations denoted by natural selection do constitute an operating cause in the evolution of species. Only, he goes deeper: he, with his characteristic truly philosophic insight, sees in natural selection a *proximate* cause; sees behind it the primordial operations of forces of nature which rendered natural selection possible, and supplied it with a *point-d’appui*. Then he assigns use and disuse as another cause in the origination of species. Now all this is not a “declaration against” what your correspondent pleases to call “Mechanical Philosophy,” but is a part and parcel of it. It is rather a “declaration against” all sorts of teleological philosophy. Let him remember also that Spencer’s philosophy is the acknowledged philosophy of evolution; and he may rest assured that, even if the theory of natural selection as a *cause* in the genesis of species be proved untrue, that philosophy will still stand opposed to any philosophy that will attempt to bring back “Mind” as one of the *causes* of organic evolution.

Your correspondent is a little too hasty in his rejoicings over Mr. Romanes’ paper on “Physiological Selection.” He will see from the second part of the paper that even Mr. Romanes is unable to deny that in some cases at least natural selection is quite competent to originate species.

Then your correspondent thinks the theory of natural selection “not only essentially faulty and incomplete, but fundamentally erroneous,” “in so far as it assumes variations to arise by accident.” Now by “accident” or “chance” in this connection, evolutionists (including Darwin) have simply meant the action

<sup>1</sup> From the above it follows that local races or species produced by isolation should be more or less fertile with the parent form. This is a point which might well be tested experimentally.